The solar radiation to which the comet was subjected would be inadequate to account for this state of things directly. Is there chemical action set up within the comet by the sun's heat? Is the comet's light due to electricity in any form excited by the effect of the solar radiation upon the matter of the comet? Are we to look for the source of the light to the friction of the particles of the cometary matter which has been thrown into violent agitation by the comet's approach to the sun?

The comet was unfavourably situated for examination from my observatory, as it was seen on a background of sky illuminated by the lights of London; and as it approached the horizon, it became partially concealed by the chimneys of some neighbouring houses. Nearly the whole of the time that the comet was visible was consumed in the observations with the spectroscope; and a few sketches only were made of the appearances presented by the head of the comet in the telescope.

The two sketches which accompany this note (Plate 6) were made on July 13 and 14. On the latter evening the fainter parts of the coma, which are wanting in the sketch, were rendered invisible by the bright background of sky. Two of the phases presented by the intersection of the envelopes are shown in the sketches. The narrow black channel behind the nucleus passed on the right-hand side of the nucleus (as seen in the telescope), where it terminated in a small round extension, presenting something of the appearance of a black pin, with its head by the side of the nucleus.

III. "On Acoustic Reversibility." By J. Tyndall, D.C.L., LL.D., F.R.S. Received December 31, 1874.

On the 21st and 22nd of June, 1822, a Commission appointed by the Bureau of Longitudes of France executed a celebrated series of experiments on the velocity of sound. Two stations had been chosen, the one at Villejuif, the other at Montlhéry, both lying south of Paris, and 11·6 miles distant from each other. Prony, Mathieu, and Arago were the observers at Villejuif, while Humboldt, Bouvard, and Gay-Lussac were at Montlhéry. Guns, charged sometimes with 2 lbs. and sometimes with 3 lbs. of powder, were fired at both stations, and the velocity was deduced from the interval between the appearance of the flash and the arrival of the sound.

On this memorable occasion an observation was made which, as far as I know, has remained a scientific enigma to the present hour. It was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness at Villejuif, by far the greater number of the reports from Villejuif failed to reach Montlhéry. Had wind existed, and had it blown from Montlhéry to Villejuif, it would

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have been recognized as the cause of the observed difference; but the air at the time was calm, the slight motion of translation actually existing being from Villejuif towards Montlhéry, or against the direction in which the sound was best heard.

So marked was the difference in transmissive power between the two directions, that on the 22nd of June, while every shot fired at Montlhéry was heard "à merveille" at Villejuif, but one shot out of twelve fired at Villejuif was heard, and that feebly, at the other station.

With the caution which characterized him on other occasions, and which has been referred to admiringly by Faraday*, Arago made no attempt to explain this anomaly. His words are:—"Quant aux différences si remarquables d'intensité que le bruit du canon a toujours présentés suivant qu'il se propageaient du nord au sud entre Villejuif et Montlhéry, ou du sud au nord entre cette seconde station et la première; nous ne chercherons pas aujourd'hui à l'expliquer, parce que nous ne pourrions offrir au lecteur que des conjectures denuées de preuves" †.

I have tried, after much perplexity of thought, to bring this subject within the range of experiment, and have now to submit to the Royal Society a possible solution of the enigma. The first step was to ascertain whether the sensitive flame referred to in my recent paper in the Philosophical Transactions could be safely employed in experiments on the mutual reversibility of a source of sound and an object on which the sound impinges. Now the sensitive flame usually employed by me measures from 18 to 24 inches in height, while the reed employed as a source of sound is less than a square quarter of an inch in area. If, therefore, the whole flame, or the pipe which fed it, were sensitive to sonorous vibrations, strict experiments on reversibility with the reed and flame might be difficult, if not impossible. Hence my desire to learn whether the seat of sensitiveness was so localized in the flame as to render the contemplated interchange of flame and reed permissible.

The flame being placed behind a cardboard screen, the shank of a funnel passed through a hole in the cardboard was directed upon the middle of the flame. The sound-waves issuing from the vibrating reed placed within the funnel produced no sensible effect upon the flame. Shifting the funnel so as to direct its shank upon the root of the flame, the action was violent.

To augment the precision of the experiment, the funnel was connected with a glass tube 3 feet long and half an inch in diameter, the object being to weaken by distance the effect of the waves diffracted round the edge of the funnel, and to permit those only which passed through the glass tube to act upon the flame.

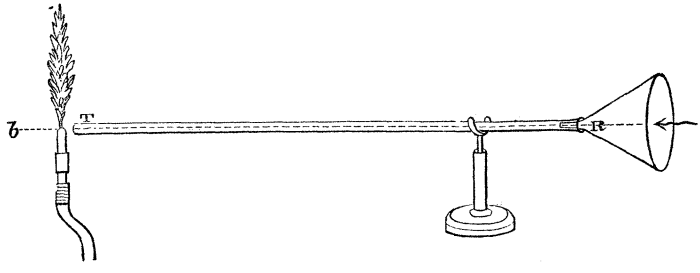
Presenting the end of the tube to the orifice of the burner (*b*, fig. 1),

* Researches in Chemistry and Physics, p. 484.

† Connaissance des Temps, 1825, p. 370.

or the orifice to the end of the tube, the flame was violently agitated by the sounding-reed, R. On shifting the tube, or the burner, so as to concentrate the sound on a portion of the flame about half an

Fig. 1.



inch above the orifice, the action was *nil*. Concentrating the sound upon the burner itself about half an inch below its orifice, there was no action.

These experiments demonstrate the localization of "the seat of sensitiveness," and they prove the flame to be an appropriate instrument for the contemplated experiments on reversibility.

The experiments proceeded thus:—The sensitive flame being placed close behind a screen of cardboard 18 inches high by 12 inches wide, a vibrating reed, standing at the same height as the root of the flame, was placed at a distance of 6 feet on the other side of the screen. The sound of the reed, in this position, produced a strong agitation of the flame.

The whole upper half of the flame was here visible from the reed; hence the necessity of the foregoing experiments to prove the action of the sound on the upper portion of the flame to be *nil*, and that the waves had really to bend round the edge of the screen so as to reach the seat of sensitiveness in the neighbourhood of the burner.

The positions of the flame and reed were reversed, the latter being now close behind the screen, and the former at a distance of 6 feet from it. The sonorous vibrations were without sensible action upon the flame.

The experiment was repeated and varied in many ways. Screens of various sizes were employed; and instead of reversing the positions of the flame and reed, the screen was moved so as to bring, in some experiments the flame, and in other experiments the reed, close behind it. Care was also taken that no reflected sound from the walls or ceiling of the laboratory, or from the body of the experimenter, should have any thing to do with the effect. In all cases it was shown that the sound was effective when the reed was at a distance from the screen and the flame close

behind it; while the action was insensible when these positions were reversed.

Thus, let *s e*, fig. 2, be a vertical section of the screen. When the reed was at A and the flame at B there was no action; when the reed was at B

Fig. 2.

and the flame at A the action was decided. It may be added that the vibrations communicated to the screen itself, and from it to the air beyond it, were without effect; for when the reed, which at B is effectual, was shifted to C, where its action on the screen was greatly augmented, it ceased to have any action on the flame at A.

We are now, I think, prepared to consider the failure of reversibility in the larger experiments of 1822. Happily an incidental observation of great significance comes here to our aid. It was observed and recorded at the time that while the reports of the guns at Villejuif were without echoes, a roll of echoes, lasting from 20 to 25 seconds, accompanied every shot at Montlhéry, being heard by the observers there. Arago, the writer of the Report, referred these echoes to reflection from the clouds, an explanation which I think we are entitled to regard as problematical. The report says that "tous les coups tirés à Montlhéry y étaient *accompagnés* d'un roulement semblable à celui du tonnerre." I have italicized a very significant word—a word which fairly applies to our experiments on gun-sounds at the South Foreland, where there was no sensible solution of continuity between explosion and echo, but which could hardly apply to echoes coming from the clouds. For supposing the clouds to be only a mile distant, the sound and its echo would have been separated by an interval of nearly ten seconds. But there is no mention of any interval; and had such existed, surely the word "followed," instead of "accompanied," would have been the one employed. The echoes, moreover, appear to have been *continuous*, while the clouds observed seem to have been *separate*. "Ces phénomènes," says Arago, "n'ont jamais eu lieu qu'au moment de l'apparition de quelques nuages." But from separate clouds a continuous roll of echoes could hardly come. When to this is added the experimental fact that clouds far denser than any ever formed in the atmosphere are demonstrably incapable of sensibly reflecting sound, while cloudless air, which Arago pronounced echoless, has been proved capable of powerfully reflecting it, I think we have

strong reason to question the hypothesis of the illustrious French philosopher.

And considering the hundreds of shots fired at the South Foreland, with the attention specially directed to the aerial echoes, when no single case occurred in which echoes of measurable duration did not accompany the report of the gun, I think Arago's statement that at Villejuif no echoes were heard when the sky was clear must simply mean that they vanished with great rapidity. Unless the attention were specially directed to the point, a slight prolongation of the cannon-sound might well escape observation; and it would be all the more likely to do so if the echoes were so loud and prompt as to form apparently part and parcel of the direct sound.

I should be very loth to transgress here the limits of fair criticism, or to throw doubt, without good reason, on the recorded observations of an eminent man. Still, taking into account what has been just stated, and remembering that the minds of Arago and his colleagues were occupied by a totally different problem (that the echoes were an incident rather than an object of observation), I think we may justly consider the sound which he called "instantaneous" as one whose aerial echoes did not differentiate themselves from the direct sound by any noticeable fall of intensity, and which rapidly died into silence.

Turning now to the observations at Montlhéry, we are struck by the extraordinary duration of the echoes heard at that station. At the South Foreland the charge habitually fired was equal to the largest of those employed by the French philosophers; but on no occasion did the gun-sounds produce echoes approaching to 20 or 25 seconds' duration. It rarely reached half this amount. Even the syren-echoes, which were more remarkable and more long-continued than those of the gun, never reached the duration of the Montlhéry echoes. The nearest approach to it was on the 17th of October, 1873, when the syren-echoes required 15 seconds to subside into silence.

On this same day, moreover (and this is a point of marked significance), the transmitted sound reached its maximum range, the gun-sounds being heard at the Quenocs buoy, which is $16\frac{1}{2}$ nautical miles from the South Foreland. I have already stated that the duration of the air-echoes indicates "the atmospheric depths" from which they come*. An optical analogy may help us here. Let light fall upon chalk, the light is wholly scattered by the superficial particles; let the chalk be powdered and mixed with water, light reaches the observer from a far greater depth of the turbid liquid. The solid chalk typifies the action of exceedingly dense acoustic clouds; the chalk and water that of clouds of moderate density. In the one case we have echoes of short, in the other echoes of long duration. These considerations prepare us for the inference that Montlhéry, on the occasion referred to, must have been sur-

* Phil. Trans. 1874, pt. i. p. 202.

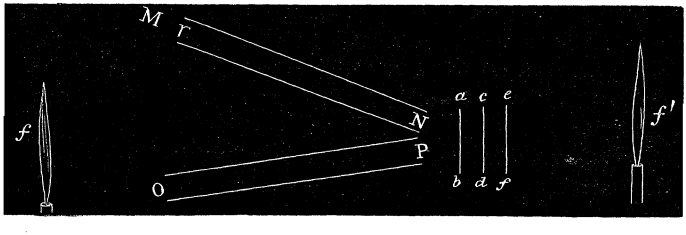
rounded by a highly diacoustic atmosphere; while the shortness of the echoes at Villejuif shows the atmosphere surrounding that station to have been acoustically opaque.

Have we any clue to the cause of the opacity? I think we have. Villejuif is close to Paris, and over it, with the observed light wind, was slowly wafted the air from the city. Thousands of chimneys to windward of Villejuif were discharging their heated currents; so that an atmosphere non-homogeneous in a high degree must have surrounded that station. At no great height in the atmosphere the equilibrium of temperature would be established. The non-homogeneous air surrounding Villejuif is experimentally typified by our screen with the source of sound close behind it, the upper edge of the screen representing the place where equilibrium of temperature was established in the atmosphere above the station. In virtue of its proximity to the screen, the echoes from our sounding-reed would, in the case here supposed, so blend with the direct sound as to be practically indistinguishable from it, as the echoes at Villejuif followed the direct sound so hotly, and vanished so rapidly, that they escaped observation. And as our sensitive flame, at a distance, failed to be affected by the sounding body placed close behind the cardboard screen, so, I take it, did the observers at Montlhéry fail to hear the sounds of the Villejuif gun. This is the explanation of Arago's difficulty which I have the honour to submit to the Royal Society.

Received January 4, 1875.

Something further may be done towards the experimental elucidation of this subject. The facility with which sounds pass through textile fabrics has been already illustrated*, a layer of cambric or calico, or even of thick flannel or baize, being found competent to intercept but a fraction of the sound from a vibrating reed. Such a layer of calico may be taken to represent a layer of air differentiated from its neighbours by temperature or moisture; while a succession of such sheets of calico may be taken to represent successive layers of non-homogeneous air.

Fig. 3.



Two tin tubes (M N and O P, fig. 3) with open ends are placed so

* Phil. Trans. 1874, pt. i. p. 208.

as to form an acute angle with each other. At the end of one is the vibrating reed r; opposite the end of the other, and in the prolongation of P O, is the sensitive flame f, a second sensitive flame (f') being placed in the continuation of the axis of M N. On sounding the reed, the direct sound through M N agitates the flame f'. Introducing the square of calico ab at the proper angle, a slight decrease of the action on f' is noticed, and the feeble echo from ab produces a barely perceptible agitation of the flame f. Adding another square, cd, the sound transmitted by ab impinges on cd; it is partially echoed, returns through ab, passes along P O, and still further agitates the flame f. Adding a third square, ef, the reflected sound is still further augmented, every accession to the echo being accompanied by a corresponding withdrawal of the vibrations from f' and a consequent stilling of that flame.

With thinner calico or cambric it would require a greater number of layers to intercept the entire sound; hence with such cambric we should have echoes returned from a greater distance, and therefore of greater duration. Eight layers of the calico employed in these experiments, stretched on a wire frame and placed close together as a kind of pad, may be taken to represent a very dense acoustic cloud. Such a pad, placed at the proper angle beyond N, cuts off the sound, which in its absence reaches f', almost as effectually as an impervious solid plate*: the flame f' is thereby stilled, while f is far more powerfully agitated than by the reflection from a single layer. With the source of sound close at hand, the echoes from such a pad would be of insensible duration. Thus close at hand do I suppose the acoustic clouds surrounding Villejuif to have been, a similar shortness of echo being the consequence.

A further step is here taken in the illustration of the analogy between light and sound. Our pad acts chiefly by internal reflection. The sound from the reed is a composite one, made up of partial sounds differing in pitch. If these sounds be ejected from the pad in their pristine proportions, the pad is acoustically *white*; if they return with their proportions altered, the pad is acoustically *coloured*.

In these experiments my assistant, Mr. Cottrell, has rendered me material assistance.

* January 13th.—Since this was written I have sent the sound through fifteen layers of calico, and echoed it back through the same layers, in strength sufficient to agitate the flame. Thirty layers were here crossed by the sound.

Fig. 3.